

SCIENTIFIC PEER REVIEW: A CASE STUDY FROM LOCAL AND GLOBAL ANALYSES

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The dialog between Staddon (2001, 2004) and Baum (2004) raises general questions about the nature of scientific peer review. Their dialog displays effects on peer review of differences of opinion about the relative merits of local and global analyses. Baum (1995, 1997, 2001, 2002) favors global analyses as a paradigm different, newer, and better than the local, dynamic, real-time approach that plays a significant role in Staddon (2001). According to the Kuhnian perspective (Kuhn, 1996) Baum advocates, we can better understand his review of Staddon (2001) by considering the implications for it of his commitment to the idea that a global analysis is a superior scientific paradigm. This commentary examines some characteristics of local and global analyses, as well as some of their possible implications for peer review in the context of a reviewer's belief in the Kuhnian idea of incommensurability: According to this idea, a reviewer who either is, or who believes he is, from one paradigm is unlikely, for better or worse, to understand or perhaps even tolerate work from a different paradigm. It is recommended that a process be developed to encourage "truth in peer reviewing" to reduce possible conflicts of interest embedded in the current conception of scientific peer review.

Key words: scientific peer review, local analyses, global analyses, molar, molecular, behaviorism

Paradigms and Peer Review

Peer review is commonly assumed to be part of the foundation of modern science. It is seen as an essential tool by which good science is discriminated from bad science, even though there is no scientific theory of scientific peer review. We have bits and pieces of such a theory in the form of research on persuasion, effective communication, the meaning of novelty, the relation between science and culture, the formation and maintenance of groups with shared views, how beliefs shape perception, and theories of science.

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The scientific community relies on this incompletely understood process of peer review to determine the difference between good and bad science without having anything remotely resembling the kind of theory to explain it that is demanded of far less important scientific projects. Science, that is, does not understand itself by its own scientific standards to the extent to which it relies on peer review to determine its own development. It is not surprising, therefore, that peer review sometimes becomes awkward and sometimes reminds us of the arbitrary traditions and poorly understood conventions that often define it. The dialog between Staddon (2001, 2004) and Baum (2004) in this sense raises challenging and important questions about peer review and hence about science itself.

The two scientists differ on two key themes: (a) the scientific usefulness of local ("molecular") and global ("molar") levels of analysis, and (b) the usefulness of contextualism in behavioral science. On the one hand, Staddon (1964, 1968, 2001), over the course of his career, has often conducted dynamic, real-time, local analyses to clarify behavioral processes and has for some time expressed doubts about contextualism, especially about its most radical forms that tolerate different realities (e.g., Staddon, 1993). Baum, on the other hand, has used aggregate and usually static analyses and has advocated the use of contextualism to better understand behavior analy-

sis (Baum, 1994, 1997, 2002). Baum (1994, 2002) has suggested that the difference between local and global approaches defines a paradigm clash (Kuhn, 1996) and has recently suggested that local and global analyses define reality differently (Baum, 2002). Claims that different paradigms define different realities are usually associated with some of the most interesting, and also most radical, interpretations of what Kuhn meant by a "paradigm shift," such as that reality changes after a paradigm shift. These differences between Staddon and Baum are big differences. They are so big that it would appear useful to consider how they might affect how one sees the other's science.

My goal in this commentary is to shed light on the dialog between Staddon and Baum. Their dialog cannot be understood, in my judgment, without considering the implications of Baum's belief in a paradigm clash between local and global analyses, because theory evaluation and hence peer review take on, according to the Kuhnian view Baum advocates, different characteristics in the context of a paradigm clash (Kuhn, 1996; Hanson, 1969). I will therefore first review what Kuhn meant by a paradigm clash and then briefly address part of Baum's argument for why the distinction between local and global behavioral analyses satisfies the definition of a paradigm clash. This discussion deals with the legitimacy of Baum's claim that the molar/molecular issue exemplifies Kuhnian ideas, not with the legitimacy, or lack thereof, of Kuhn's account of the nature of science in the first place. Finally, I will consider possible implications for peer review and make two recommendations.

A Vision of an Emerging Paradigm Clash

What is a paradigm clash? Kuhn (1996) defined a paradigm clash in terms of his vision of the nature of science. He asserted that a careful historical analysis of major conceptual shifts in science, such as that from a geocentric to a heliocentric view of the solar system, from a phlogiston-based theory of fire to the discovery of oxygen and contemporary accounts of combustion, from Newtonian physics to relativistic physics, and so on, revealed a critical feature of science: There are two phases in science, normal science conducted within a paradigm and revolutionary science

conducted when a paradigm is in crisis or during a paradigm shift. The former describes how science works if a scientific community agrees on a paradigm, when most scientists spend most of their time articulating the current paradigm, and the latter applies if there is no agreed upon paradigm and scientists spend much of their time challenging the fundamentals of each others' approaches. A crisis may or may not be resolved by a paradigm shift. A crisis may be brought on by such events as a scientific research community's beginning to view a conventional, not yet solved problem within an older paradigm as a critical anomaly that cannot in principle be solved by it, rather than as a conventional puzzle the ultimate solution to which feels guaranteed by the older paradigm. During a crisis, scientists act confused, no longer agree on basic concepts, on appropriate methods, or on what empirical phenomena are basic. A crisis may be resolved by a scientific revolution if an alternative paradigm emerges and it appears to provide, at least in principle, a solution to the anomaly.

According to Kuhn (1996), this anomaly may take the form of a small, otherwise relatively uninteresting and esoteric measurement. The crisis is therefore not necessarily resolved by data the older paradigm would have considered vitally important. If a crisis is resolved by a scientific revolution, so that there is a paradigm shift, basic concepts are changed, key phenomena to be explained are changed, empirical methods are changed, and, to preserve what to Kuhn seemed only the illusion of cumulative progress in science, history comes to be rewritten so that the previous paradigm comes to be thought of as having been an earlier, special case, of the current paradigm; that is, comes to be incorrectly thought of as having conformed to the present paradigm. Kuhn stated that after a paradigm shift, scientists live in a different world, and in the most radical interpretations of what Kuhn meant by this, reality is said to change after a paradigm shift.

A paradigm is like a game in the sense that it guarantees to the players that if they spend enough time and energy playing it, they will solve the important problems the paradigm defines, even though the details of how to solve the problems may still be unclear: A purchaser of a jigsaw puzzle feels guaranteed

that if he tries hard enough, the puzzle will ultimately be solved. A paradigm is not like a game, however, in the sense that games are well defined and a paradigm is not. Kuhn stated that a paradigm always involves unaware theoretical commitments and unexamined assumptions. Kuhn adopted a rather old literature on the effects on visual perception of distorting prisms: He took this literature to mean that if a person wore distorting prisms for a sufficiently long time in real-world settings the world would begin to look normal. According to this position, any scientist who is committed to a paradigm, and who has learned to see the world through its idiosyncratic distorting prisms, believes the paradigm's distorted world is the real world.

According to Kuhn, a paradigm in this sense always involves arbitrary tradition and convention of which its advocates are unaware. Kuhn stated that because historical analysis shows that science has always been like this, science presumably still is like this and always will be like this. He indicated that we could therefore expect to experience scientific revolutions indefinitely into the future.

Baum suggests there is a paradigm clash between local and global analyses. Baum (2002) suggests that the contrast between global and local analyses exemplifies a Kuhnian paradigm clash, and he appears to adopt the exceedingly interesting and radical position that the two paradigms have different ontological implications, that is, define reality differently. Others have interpreted the difference between global and local accounts in terms of Kuhn's historicist position (e.g., Shimp, 1984; 2001), but Baum is among those who appear to adopt an extreme interpretation of Kuhn's position, according to which reality itself is different after a paradigm shift. At least Baum (2002) does not disambiguate his claim that the two paradigms define reality differently from the position that after a paradigm shift, which he advocates, reality is different. Many researchers interested in the science of behavior have expressed concerns about this extreme position (e.g., Shimp, 2001; Staddon, 1993; Zuriff, 1985), which is perhaps most familiar in literary deconstructionism, but which is seldom seriously encountered within the scientific community. Baum, however, challenges the physical reality of real-time,

momentary behaviors that often play a role in local analyses: From Baum's global point of view, these behaviors do not exist (Baum, 2002), and he also challenges the scientific utility of the concept of the behavior stream, with its corresponding notions related to the continuous change in behavior through time (Baum, 2002). Thus it appears that he is deadly serious when he writes that the difference between local and global analyses represents an extreme form of a Kuhnian paradigm clash and that there should be a paradigm shift from local analyses to global analyses.

According to the radical interpretation of Kuhn's theory Baum adopts, local and global paradigms are incommensurably different: They cannot be evaluated in terms of shared evaluative standards. Therefore, a scientific community committed to one paradigm may find it virtually impossible to understand or to have a constructive dialog with the community committed to the other one. In short, if Baum is correct that there is a paradigm clash between local and global analyses, then he would find it difficult to understand Staddon's book to the extent to which its arguments depend on local analyses, such as the leaky integrator model (Staddon & Higa, 1996) with which Staddon illustrates his kind of theoretical behaviorism.

Truth in advertising and truth in peer reviewing. Truth in advertising is designed to alert people to the possibility of a conflict of interest so that they can better understand how to interpret a message. If we read that an exotic herb cures old age, but the writer discloses that he owns the company that sells the herb, we interpret the claim accordingly. Or, if we read that a new book is the most pedagogically effective philosophy text ever written, but the claim is in the context of an advertisement for the company that publishes the book, again we interpret the claim accordingly. We tolerate that different people have different opinions about herbal cures for old age or about effective philosophy texts. We simply feel that readers need to be advised about the source of claims made about cures and texts. Science functions somewhat differently. The point of science is usually taken to be understanding the natural world, and it is almost universally assumed, apart from relativistic accounts such as extreme interpreta-

tions of Kuhn (1996), that there is only one natural world. There are no alternatives as with herbal cures or philosophy texts. Bio-medical researchers are increasingly asked to inform editors or readers about any possible financial self-interest that might cloud their scientific judgment; but more often science simply assumes that there is one truth and that researchers who believe they know it should try to ensure their view wins over competing and incorrect views, rather in a manner reminiscent of how competitive sports are played or how our legal system encourages fighting to win. Scientists are assumed to be dispassionate seekers of the one truth, and are therefore not usually required to disclose intellectual self-interest or intellectual biases. Science assumes either that there are no such biases or that if there are, the process of peer review and the more general scientific method help to guarantee a self-correcting process so that ultimately truth will win over error.

Some scientists sometimes believe other scientists occasionally violate pure scientific objectivity. A kind of scientific analog of truth in advertising therefore might seem helpful. What if, for example, there was some kind of mechanism within the scientific process by which researchers and reviewers could disclose a potential bias or self-interest they felt their papers might reflect? Might then peer review be more nearly objective, in the sense that a reader would be better informed about a reviewer's biases? From Kuhn's point of view, however, even such an otherwise helpful process might pose insuperable difficulties, because scientists would not be aware of their own biases, or distorting prisms, at least not in the more radical forms of Kuhn's theory.

What then is to be done? If Baum (2002) is correct, that the local/global difference is a paradigm clash, then not only Baum is seeing through distorting prisms, but so is everyone else, and no one knows how his distorting prisms affect his scientific judgment. According to this view, Staddon, I, and any other commentators on their dialog, as well as any readers, have the same limitation. I personally find this view ultimately unsatisfactory and even depressing, despite the undoubtedly exciting intellectual and cultural clashes it would produce. I therefore personally prefer an interpretation of Kuhn's book that is less radical than Baum's. I prefer the

view Kuhn himself expressed in his postscript, in which he denied ever having implied the most radical view. In the postscript, he suggested that researchers could overcome the communication difficulties and obstacles thrown up by different distorting prisms in paradigm clashes if they were only to work hard enough at it. I see communication problems as due more to remediable issues in scientific training and culture than to the existence of different realities.

Therefore, difficult or impossible as it may be, I would like nevertheless to try to shed light on the difference between Staddon's and Baum's general perspectives, and therefore on the nature of their dialog, by tentatively proposing some possible prisms that might be operating here. Imagine a 2×2 table, with one dimension being a scientist's expressed preference for local or global analyses and with the other being the scientist's expressed preference for positivistic or contextualist analyses (see also Shimp, 2004). In terms of this table, we might characterize a scientist as being, for example, a local positivist; that is, as one who likes real-time behavioral analyses but who dislikes Kuhn's theory of science. Scientists, however, are people, and people do not always describe their own activities in ways others find descriptively accurate. So, we might even imagine two forms of this table, one to describe how a scientist characterizes himself and one to describe how others see him.

In terms of the version categorizing a researcher's self-description, I understand Baum to say he is a global contextualist. On the one hand, he favors global analyses and advocates a Kuhnian view. On the other hand, for whatever it is worth, I would categorize him, in terms of the other table, as a global positivist as well as a global contextualist, because his laboratory work seems to satisfy conventional positivistic standards. Staddon's own self-characterizations sound very positivistic, and he conducts both local and global laboratory analyses. Thus, in terms of the first table, he seems to fall into two cells of the table, both local and global positivism. Just as I see Baum's research as being categorized differently in the two different tables, corresponding to what he says about himself versus what others might say, I see Staddon's conceptual analyses as being much

more contextualist, or Kuhnian, than he apparently does. Thus I strongly endorsed his paper with Higa on timing (Shimp, 1999; Staddon & Higa, 1999), in part precisely because it gave the kind of account of the historical and conceptual development of a theory of timing that a Kuhnian might give (e.g., Pickering, 1984, for quarks, and Hanson, 1963, for positrons). I would therefore categorize important features of Staddon's work as contextual, in contrast to his own self-characterizations. In short, different individuals have different impressions of their own as well as of other individuals' research, and this is the kind of thing that can itself be seen either as evidence for Kuhn's distorting-prism metaphor or as evidence that communication between scientists may be difficult in the best of times, because after all, both Staddon and Baum are members of a relatively small and relatively homogeneous scientific community and share a great deal of intellectual history and training.

Having said all of the above, it clearly behooves me to alert the reader to how I think my own views are probably distorted. Traditional scientific writing style, as well as personal inclination, would ordinarily lead me to see this kind of self-description as narcissistic, pompous, and ridiculous, but the logic of truth in advertising seems to require me, as a commentator on peer review, to engage in some uncomfortable self-disclosure. Like Baum, I have suggested that historical and conceptual analyses similar to Kuhn's facilitate understanding how science works (Shimp, 1999; 2001), and I have suggested that social constructionism, an intellectual movement inspired in part by Kuhn's work, may facilitate understanding behavior analysis (Shimp, 2001). I enjoy considering the conceptual and empirical implications of Gestalt ideas about perceptual organization (Herbranson, Fremouw, & Shimp, 1999; 2002), levels of perceptual analysis (Fremouw, Herbranson, & Shimp, 1998, 2002), and reversible figures for the nature of scientific knowledge. All this sounds Kuhnian. I remind the reader, however, that Kuhn himself sometimes disavowed the radical position Baum and others attribute to him, and he encouraged prolonged dialogs between members of seemingly opposed research communities in order to improve mutual un-

derstanding and, presumably, respect. I applied Kuhnian ideas to Williams (1990) and concluded, much as I do in this commentary, that peer review can be little more than an indirect reflection of a reviewer's own personal beliefs (Shimp, 1990). I vary at times with respect to the version of constructionism to which I feel committed, but increasingly feel more contextualist and less positivist. In terms of the local/global distinction, I have tended over the years to advocate a local perspective that emphasizes the unity of extended behavioral patterns and that sees the local temporal organization of behavior as especially diagnostic of underlying psychological processes. I found this view in Staddon's book, especially in his discussion of local, real-time, dynamic models. In contrast to Baum, who believes the local/global difference defines a paradigm clash, I, in recent years (Hawkes & Shimp, 1998; Shimp, 1999), have advocated a position according to which behavioral science needs both local and global levels, not simply one or the other.

One might conclude from my self-report that I might be (a) sympathetic to Baum's Kuhnian analysis, (b) doubtful however that the local/global difference illustrates the most extreme Kuhnian position, (c) sympathetic to Staddon's dynamic local research, and (d) sympathetic to Staddon's propensity to provide historical and conceptual perspective on theoretical disputes. I will leave to the reader to judge if this prediction is approximately borne out by this commentary. To the extent to which I am correct, I am inclined to believe that when I function as a peer reviewer, it might benefit reviewees, editors, and general readers, to know what I think my biases are, just as a consumer of alternative medicines might benefit from knowing that a biomedical researcher has a financial stake in an herb described in a paper he has written.

Features of Local and Global Analyses: Do They Define a Paradigm Clash?

If Baum is right, that there is an emerging paradigm clash between global and local analyses, then a global analysis should display features that either have no local equivalents or contrast so sharply with them that it is debatable that they are indeed equivalents. If there is no paradigm clash, however, and if the differences between local and global anal-

yses are ordinary and even complementary in nature, then features of global analyses should have fairly straightforward local equivalents. In the former case, we should expect to find it difficult for a global theorist to see virtue in a local analysis, and vice versa. In the latter case, effective communication between local and global theorists might be difficult but not impossible. Therefore, we next take a look to see if we can find any plausible local analogs of features of Baum's global view. To do this, we need first to review briefly what some of the differences between local and global analyses are.

What is all the fuss about? What are local and global analyses? It is possible in this commentary to provide only the merest hint of the full range of views on the difference between local and global analyses. To gain greater perspective, in order to reduce possible distorting effects of just my view, a reader might wish to consult diverse examples, a few of which include Baum (1994, 1995, 2001, 2002); Cleaveland (1999); Dinsmoor (2001); Hawkes and Shimp (1998); Herrnstein (1961, 1970); Herrnstein and Vaughan (1980); Heyman and Tanz (1995); Hineline (2001); Hineline, Silberberg, Ziriaux, Timberlake, and Vaughan (1987); Hinson and Staddon (1983a, 1983b); Horner (2002); Horner, Staddon, and Lozano (1997); MacDonall (1998, 2000); Nevin (1969); Peele, Casey, and Silberberg (1984); Rachlin (1994, 2000); Rachlin and Laibson (1997); Reed, Soh, Hildebrandt, DeJongh, and Shek, (2000); Reid, Chadwick, Duhham, and Miller (2001); Shimp (1966, 1976b, 1992); Silberberg and Ziriaux (1985); Silva, Pear, Tait, and Forest (1996); Staddon (1964, 1968, 2001); Vaughan (1981); Wearden and Clark (1989); and Williams (1990, 1991).

I see global analyses as perfectly legitimate tools for defining and answering certain classes of questions, especially questions about everyday life. Baum (2002, p. 110) states, for example, that global accounts are better than local ones in that they more closely "resemble . . . the way people actually talk about their lives . . ." I will give three real-world examples of my own to illustrate what I mean, in order to facilitate being corrected by global theorists. Does Ellen act rationally or impulsively when she allocates enormous amounts of her personal resources, time, en-

ergy, and money preparing holiday dinners for her family instead of attending to the concerns of her staff at her job, where overwork has led several of them to quit and others to threaten to quit? In this case, we might not be interested in the local temporal processes in individual cases of Ellen's preparing dinner: The question as posed does not seem to demand that we look at the local structure of Ellen's behavior. Or, what does it tell us about Abe and his mother, where Abe is a 13-year-old student of political theory, when he says, after she asks him to tidy up his room, "This is a free country, and I can do anything I want"? In this case, the behavior is partly the actual verbal statement that may consume only a very short time and is partly a history of parent-child interactions extended over years. I see a legitimate global component in this example in the form of questions such as how Abe allocates time and energy, say over a year, to verbally defending what he sees as his personal freedom. A local component might consist of the particular syntax of particular verbal statements. Note that like the global component, the local component might involve a long environmental history: The difference between local and global analyses is not that one involves effects of environmental history and the other does not. (Neither is it that one involves aggregates and the other does not: Many local analyses involve averages, but chiefly as aids to uncovering local processes, not as ends in their own right.) A third example might be: Why does Horace routinely undermine the reputations of his coworkers by spreading misrepresentations about them in the hopes of acquiring some of the power he intends for them to lose? It seems to me that global theorists are more likely than local theorists to ask questions such as these, and it makes perfectly good sense that they do so, because it would seem hopeless, certainly now and perhaps forever, for a local theorist to provide a complete moment-to-moment historical explanation of how the past histories of Ellen, Abe, and Horace led them to allocate time and resources to family over job, school over family, or unethical over ethical job performance.

However, it should not be assumed that simply because one does not have an organism's complete environmental history that a local analysis is impossible and that only an

aggregate analysis makes sense. Consider the following. If local theorists are unlikely to address questions such as those just described, global theorists are unlikely to address such questions as: How can Ellen divide attention among the four dishes she is now preparing for dinner so that they all will be ready within one minute of each other? Should she keep checking the scallops in the sauté pan so they don't overcook and ignore the pot roast, or should she also occasionally check the mushrooms she just added to the pot roast? Could Abe's mother encourage him to say something more civilized in the next one second by giving him a supportive smile? Could Horace's likelihood of misleading a colleague in a business meeting be reduced by reminding him beforehand of what happened four months ago when he misled a colleague and eventually was reprimanded by his boss? In general, I'm inclined to agree with Baum (2002) that local analyses tend to derive from a view that has been basic to much of modern science, and certainly to much of experimental psychology. The view in psychology has often been manifested in questions about "mental chronometry" and related questions about the local dynamics of learning, memory, categorization, attention, and motor control. Thus the world of behavior is large and local and global theorists often look at different behavioral phenomena and look at different possible types of explanations. In this sense, local and global analyses are clearly complementary.

A dimension along which global and local accounts may be diverging is in terms of the degree to which they are designed to deal with everyday phenomena expressed in plain English. Baum (2002) states it is a virtue of global accounts that they can and do deal with such behavior, and indeed several applications of the matching law have been to real world phenomena, including everyday monetary decisions, everyday criminal behavior, everyday self-control, and so on. Relatively few local analyses have been of this type and instead follow the traditional scientific tradition, well described by Kuhn (1996), according to which science develops in a way so that it diverges ever further away from ordinary, everyday human experience. Thus a contemporary particle physicist would not see it as an advantage for Aristotle's theory of the ba-

sic elements of the natural world, including things like earth, air, fire, and water, that everyone can describe them in plain English and experience them on an everyday basis.

One might think that local and global theorists would have ample work simply trying to answer the questions their own analyses define, and that there would be peace between them. However, sometimes both local and global analyses make predictions about the same behavior in the same situation, and in that case, fights sometimes break out. In that case, researchers on different sides have tried to show their side is right and the other side is wrong. Perhaps the single most common battle ground involves an experimental chamber, a hungry pigeon pecking two keys for food, and food that is delivered for pecks according to various kinds of probabilistic rules. Global theorists have interpreted the resulting behavior in terms of functional relations between aggregates, like mean rate of pecking a key, or mean time allocated to a response alternative, and mean rate of reinforcement for pecking the keys. Local theorists interpret the behavior in terms of local functions, like the likelihood of a peck on one key as a function of the local ratio of times since the most recent pecks on the two keys (Hinson & Staddon, 1983a, 1983b). In this case, there are fairly simple litmus tests that help identify whether an analysis is local or global. One litmus test consists of a scientist's view of the Matching Law (Herrnstein, 1961, 1970; Rachlin & Laibson, 1997) that describes a set of relations among various behaviors and rewards. If one sees it as an actual law rather than as a summary description of a limited kind of data from a restricted range of tasks, then there is a good chance one is a global theorist. Alternatively, if one sees it as derived from underlying dynamic processes that so happen to combine in certain tasks to coincidentally produce matching, then there is a better chance a person is a local theorist. A very old but still somewhat of a defining example consists of the difference between Baum and Rachlin (1969), Herrnstein (1961, 1970), and Nevin (1969) on the one hand, who gave global views of matching, and Shimp (1966, 1992), Silberberg and Zirrax (1985), and Hinson and Staddon (1983a, 1983b) on the other hand, who gave local accounts. This simple litmus test, like any other

single test, is not universally applicable because, as the list of shared features described below shows, there is considerable overlap between local and global analyses, or so it seems to me.

An alternative to this either/or approach is that of Hineline (2001) and Hawkes and Shimp (1998) who suggested *both* local and global approaches are necessary and the question is really to what tasks and to what behaviors does each apply, rather than which is right and which is wrong. From this perspective, local and global analyses are complementary and the difference between them does not define a paradigm clash.

Contiguity. “Contiguity” in the present context refers to events occurring at the same time and place. Local and global accounts are similar in that neither has much that is favorable to say about strict contiguity of stimulus, behavior, and reinforcement. Global theory sometimes describes behavior–reinforcement relations in terms not of contiguity but in terms of action-at-a-distance in physics, and I (Shimp, 1975, 1976a, 1976b, 1992) have discussed the dynamic, real-time mechanisms by which local temporal patterns of behavior can be established and maintained with only the terminal components of the patterns being contiguous with reinforcement. Interestingly, some temporal patterns examined by global theorists (Rachlin, 2000) even have about the same temporal extension as some examined by local theorists (Shimp, 1968; Staddon, 1968). The roles of contiguity in local and global analyses seem sufficiently similar that it is not easy, at least not for me, to see them as defining a paradigm clash.

Temporally extended activities and nesting. The operational definitions of many, perhaps most, behaviors that have played prominent roles in the history of behavioral science have been of very short duration. Thus we have rats pressing levers and pigeons pecking keys. According to Baum (2002), local theories address such behaviors, on behalf of a commitment to contiguity, and global theories look at behavior extended over time. In contrast, I am on record as writing that global theories often involve behaviors such as key pecks and lever presses due to their aggregating such behaviors over time. This characterization of extended performances in terms of averages obscures the local temporal structure of be-

havior, including how some local temporal patterns of behavior extending over time may be unitized (Shimp, 1975, 1976b). It would seem that each type of analysis rejects contiguity and addresses, in its own way, the issue of behavior extended over time.

Some local theories invoke behavioral processes that are continuous in time, such as short-term forgetting, which therefore have momentary states. Staddon’s leaky integrator model (Staddon, 2001) is accordingly a local model, and so are various models I have constructed (Shimp, 1992). Global theories very infrequently invoke real-time processes that generate actual behavior streams and address behavior in terms of averages over extended periods of time. One therefore might expect Baum, as a global analyst, to be less than enthralled with such models, and, in fact, Baum (2004) only says of Staddon’s model that it has appeared before and that he would be interested in reading more about such models, absent various of Staddon’s critiques of what I would call global analyses; in short, absent criticism of the view Baum favors. Baum’s performance as a reviewer seems in this sense to support his suggestion (Baum, 2002) that the difference between local and global analyses defines a paradigm clash. Whether the difference between local and global analyses in terms of the idea of extended performances defines a paradigm clash seems less clear. It might simply reflect, as I suggest above, misattributions by local and global theorists about each other’s approaches. Perhaps a topic worthy of future discussion by local and global theorists would be how to discriminate between temporally extended activities that are suitable for one kind of analysis but not the other. It is helpful to some extent to read (Baum, 2002, p. 97) that “the central ontological claim of the molar view is that behavior consists of temporally extended patterns of action.” But how extended is extended? And when is the local temporal structure of extended performance to be ignored and when is it to be seen as essential to an understanding of the extended performance? Is a pianist playing the cadenza to the first movement of Rachmaninov’s Third Piano Concerto engaged in a temporally extended action? If so, is there any sense in which the aggregate performance is independent of the temporally or-

ganized pattern of notes? A global analysis often replaces the details of a behavior stream with an aggregate summary: When would we want to characterize the cadenza in terms of its average duration, average pitch, and average loudness? I cannot think of any reasonable occasion when one might want to look at such an aggregate.

It is probably well at this point to observe that what is local and what is global is highly relative to a particular context. Just as the previous paragraph asks how global is global, one could also ask, how local is local? To a particle physicist interested in collisions between particles with the highest energy levels, the kind of extended behavior a local analyst might find of interest, extending over several seconds, would be of exceedingly long temporal extension.

Baum (2002, p. 97) states that the global view is based “on the concept of nesting, the idea that every activity (e.g., playing baseball) is composed of parts that are themselves activities.” Playing baseball would presumably then be composed of the activities, say, of swinging a bat, throwing a ball, running, and so on. Furthermore, swinging a bat at a fastball might itself be an extended action. A swing at a fastball, however, takes less time than many of the temporal patterns of behavior that are the basis for a local analysis. Again, perhaps future discussions of local and global analyses might clarify what “extended” means.

There are questions about nesting as well as about the meaning of an extended performance. There have been many discussions in the history of behavioral science about how to carve up continuous behavior into meaningful “responses.” How do we know what an extended activity is, and how do we know the nested levels within it? How do we know that a behavior belongs on one level and not another? These are all classic questions (see Anderson & Bower, 1973, for an excellent review), and neither local nor global analyses as yet have general answers. In the meantime, it is not clear to me how the difference between extended and nested activities, on the one hand, and unitized temporal patterns of responses, on the other, is so dramatic as to help define a paradigm clash.

Tempo. The idea that there is a fixed tempo to behavior is an appealing and potentially

useful simplifying idea, but there is very little supporting evidence outside of biologically fixed rhythms. Typically, even when one tries to generate arbitrary laboratory behavior that in one manner or another is statistically stable over time, local variations still can persist (e.g., Blough, 1968; Hawkes & Shimp, 1998; Shimp, 1967). The notion of a fixed tempo, when applied to behavior, minimizes the importance of local temporal organization in behavior. As in music, there is more to behavior than an average tempo. Only in the rarest cases, such as, say, Ravel’s *Bolero*, is a rhythmic pattern exactly fixed throughout a lengthy musical work. Usually, in music, speech, playing baseball, walking, or sipping Calvados, tempo is variable. To average over an entire musical performance, conversation, baseball game, walk through a mountain canyon, or evening of friendly conversation accompanied by a glass of Calvados is to lose the meaning of each of these activities. At present, however, the amount of data that can be cited to support the generality of a fixed tempo is relatively small (Hawkes & Shimp, 1998; Killeen, Hall, Scott, Reilly, & Kettle, 2002; Shull, Gaynor, & Grimes, 2001, 2002), so there is correspondingly little support for a claim that global analyses, resting on the idea of tempo, define a new kind of scientific paradigm.

Fix and sample. Many tasks are such that a particular sequence of behaviors generates higher local and/or global reinforcement than do other sequences. In some tasks, a sequence that maximizes reinforcement involves longer runs of one kind of response than of another response: An animal may stay with one response for a while, make one or a few responses of the other type, and then return to stay again for a relatively long time on the first alternative. I (Shimp, 1966) and Hinson and Staddon (1983a, 1983b) noted that that kind of sequential patterning characterized behavior maintained by one kind of task familiar in the matching law literature, and saw it as a form of locally highly adaptive behavior, given the nature of rewards for making one response versus switching to a different response. I originally described this kind of behavior in terms of “momentary maximizing,” and of a maximizing sequence, where choices tended to be to the alternative having the locally higher payoff probability.

Baum (2002) describes what seems to be a similar case where locally adaptive behavior produces what he calls a "fix and sample" pattern of behavior, and Herrnstein and Vaughan (1980) and Vaughan (1981) previously described a related hypothetical process called "melioration." If the differences among fix and sample, melioration, and momentary maximizing serve as part of the justification for a paradigm clash between local and global analyses, then some additional clarification would be most welcome. To me, they seem more similar than dissimilar, because they all involve how the local temporal organization of behavior is a function of local reinforcement conditions.

Reinforcement of behavioral patterns: Local control versus global freedom. One notable difference between local and global analyses has been the nature of the behavior on which reward is based. Local contingencies usually, but not always, involve temporal patterns, whereas global ones usually, but not always, do not. The self-control task, common in global analyses, is a counter example, and it would be of interest to interrelate the literature on self-control with that on inhibiting responding during other kinds of temporally extended patterns (Shimp, 1968; Staddon, 1968). In any case, local contingencies frequently control the real-time continuous behavior of an organism in the sense that many local contingencies demand some particular temporally organized pattern of responses if a reward is to be delivered. That requirement not only imposes more precise demands on the temporal organization of reinforced behavior, it also sets local contingencies apart from global ones on the issue of freedom. Some behavior is said to be controlled by its consequences, yet at the same time there is said to be a "free responding" situation. Local contingencies involve less freedom than global ones. Perhaps it is the "free" in "free responding" that may be a critical difference between local and global analyses. Why, however, should making reward depend on a key peck instead of on a quantitatively specified temporal pattern of, say, interkey-peck times, be of such qualitative importance that the difference contributes to a paradigm clash? Although I am inclined to think that the issue of local freedom plays a role in the dialog between Staddon and Baum, and between lo-

cal and global analyses in general, still, one has to ask, does this difference define a paradigm clash?

The relativity of "basic." Baum criticizes a contiguity-based account, which he feels is an example of a local analysis, by stating that it "fails to explain even the most elementary phenomena, such as the maintenance of moderate response rates by interval schedules of reinforcement and of high response rates by ratio schedules" (Baum, 1995, p. 15). The interesting thing about this statement is the phrase "most elementary." Is this result elementary and basic or humble and trivial? How can one know which it is? It is as though Baum believes this result plays a role in behavior analysis equivalent to that of the movement in Mercury's perihelion, in the paradigm clash in early 20th century physics. The analogy is problematic, however, because there is plausible support for a local explanation of this schedule phenomenon (Peele et al., 1984), whereas none was available for a Newtonian explanation in the planetary situation. In any case, why must the basic contrast be between a variable-interval and a variable-ratio contingency? Why not between a variable-interval contingency and a globally equivalent differential-reinforcement-of-low-rate contingency? I am guessing that researchers who might identify themselves as committed to local analyses would see the latter as at least as important as the former. This contrast in what seems basic seems generally compatible with Baum's claim that we face a paradigm clash, all the more so because it is clear that some theorists with an interest in local analyses clearly have not seen it as such an anomalous threat that it is unsolvable from within a local analysis (Peele et al., 1984). I may not be alone in believing it would be easy to generalize existing local computational models to handle what Baum sees as a basic global phenomenon (Catania, 2003).

Kuhnian Anomalies or Ordinary Puzzles?

As the Gestalt psychologists suggested, many practical problems can be viewed in multiple ways, and Kuhn (1996) adapted their example of reversible figures in his interpretation of how scientists from different paradigms view some data differently. He argued that advocates of an established paradigm might see data they cannot as yet ex-

plain as perhaps an interesting but conventional puzzle, which their paradigm is certain someday to explain. Proponents of a competing paradigm that already explains these data may see them, however, as a profound anomaly for the established paradigm, which they believe will fail to explain them. A good example of this kind of reversible figure, in the context of debates about local versus global analyses, seems to be that of the relation between a melody and its component notes. Consider the following. "Final causes may be thought of as extended patterns. Actions are explained by final causes, Rachlin (1994) explains, by fitting into them, as the notes of a tune fit into the tune" (Baum, 1997, p. 55). I find this quotation remarkable because it summarizes, in metaphorical terms, how I see local analyses. A local analysis, to me, involves determining the hierarchical structure of the local temporal organization of behavior (Shimp, 1976b). A second example is provided by Baum's elevation of the difference in response rates maintained by variable-interval and variable-ratio schedules: He sees this as an anomaly. I see it as a conventional puzzle. Examples like this are what one might expect if the difference between local and global analyses satisfies Kuhn's meaning of a paradigm clash.

Furthermore, every similarity I listed in the preceding section, between local and global phenomena or concepts, might be viewable as a reversible figure: Where I see a similarity perhaps a global theorist might see two qualitatively different things. I therefore concede it may turn out that the two indisputably different approaches are in fact so incomparably different that they define a paradigm clash. Still, I am doubtful. Is the difference really like those between heliocentric and geocentric theories, or Newtonian and relativistic physics? With all due apologies to the participants, I personally find it difficult to see such scientific grandeur in the dialog between Staddon and Baum, or in my commentary on it. I cannot bring myself to see the difference between response rates on variable-interval versus variable-ratio schedules as being as important as the movement in the perihelion of Mercury was to early 20th century physics. Paradigm shifts are very cool, and it would be exciting to participate in one, but I prefer a more humble and manageable interpretation

of what I have in the preceding section called similarities between local and global analyses. We could interpret the differences as due to inadequate scientific communication, for example. It would not take a researcher interested in the sociology of science very long to identify recent publications in one or the other tradition that do not acknowledge the existence of the other. Baum might see this as a natural consequence of a paradigm clash, but it also might be due to something else, and we may never learn what if we prematurely assume a paradigm clash. We could also interpret the differences between local and global analyses to inadequate clarification of the different kinds of problems each analysis is designed to solve, or to simple differences of opinion. Perhaps exploring these possibilities would be worthwhile before possibly prematurely closing doors on the unification of local and global analyses.

Two Recommendations for Peer Review

Peer review is different when there are big differences between a peer reviewer's beliefs and those of the researcher whose work he reviews, paradigm clash or not, as exemplified by the dialog between Staddon and Baum.

In such a complex metatheoretical context, what does it mean for a global review of a local analysis, or a local review of a global analysis, to be fair? Staddon objects that Baum's review says little about what is actually in Staddon's book, and I agree that Baum's review describes Baum's own view at least as much as it describes Staddon's book. In accordance with the Kuhnian view Baum endorses, it is not obvious from his review or from his previously published work, at least it is not obvious to me, that he cares very much about, let alone agrees with, Staddon's evaluative standards. How should peer review work in such a case?

A corresponding question is openly discussed in the arts, literature, politics, and music (Slonimsky, 1963). In these arenas, most people concede, however grudgingly, that there are legitimate alternative approaches other than their own. The question may be even more difficult to answer in science, however, than in these "softer" fields, because in science it is routinely assumed that there is one and only one truth. If a researcher be-

believes he knows truth in the form of correct scientific assumptions, beliefs, and methods, and in my experience it is a rare behavioral scientist who does not believe he knows these things, anyone who disagrees is not merely different, but wrong (Shimp, 1999, 2001). Such a scientist, acting as a peer reviewer, might even believe he had a scientific responsibility to ensure that reviewed work not conforming to his assumptions be severely criticized. To be specific, if Baum is correct that local and global analyses reflect different paradigms, then it might be exceptionally difficult for Baum to escape the distorting prisms of his global paradigm and to write a sympathetic review of the parts of Staddon's book that deal with local analyses. It is important to note that Baum's review, no matter how biased from the perspective of a local analysis, could still be a very important contribution to science in the form, for example, of an indirect description of Baum's own views.

Recommendation 1: Truth in peer reviewing should acknowledge any potential paradigm clash. On behalf of a sense of fairness to authors, editors, and readers, and on behalf of clarifying the meaning of reviews, if a reviewer believes he personally subscribes to an importantly different perspective from that of the author, he might try to find a way to say so, and to disclose a possible conflict of interest, so that everyone will have a better chance to think about the possible effects of the reviewer's Kuhnian distorting prisms through which the reviewee's work was seen. This recommendation is most needed, but perhaps least likely to be implemented, in cases where paradigm clashes exist between reviewer and reviewee. If, for example, Baum is correct that there is a local/global paradigm clash with ontological implications, then fair, accurate, unbiased local (global) peer review of global (local) analyses may be for all intents and practical purposes, impossible.

In the postscript to Kuhn's (1996) description of the way science works, he disavowed an extreme contextualist position according to which different scientific paradigms are so incommensurably different that practitioners of them are not able to communicate successfully or to have intelligible dialogs. Scientists in this extreme case might believe they have a responsibility to suppress competing,

incorrect paradigms, just as a mathematics instructor feels a responsibility to suppress errors and mistakes in his students' work. In terms of this extreme position, a paradigm shift generates a winner and a loser, a paradigm that wins comprehensively and a paradigm that loses comprehensively, as in a game of professional basketball, much of our legal system, and in wars with "unconditional surrender." Baum (2002) appears to advocate a paradigm shift of this nature, in the sense that he appears to advocate the comprehensive replacement of a local analysis with a global analysis. The implication seems to be that Baum, by his own standards, might be incapable of providing a fair review of Staddon's book. To acknowledge such possibilities might produce interestingly different, and perhaps more constructive reviews: Instead of characterizing a reviewed book as bad or incorrect in numerous ways, a reviewer might also describe the metatheoretical assumptions to which he subscribes that make it impossible for him to admire the work he is reviewing.

Recommendation 2: Local and global analyses might be viewed as mutually facilitating rather than mutually threatening. Because I have advertised at least some of my own views, perhaps I may be permitted to suggest that it might facilitate progress if we practice some form of tolerance for competing paradigms. I partly make this recommendation in recognition of the fact that one's own personal belief about a paradigm, no matter how strongly held, may be sadly mistaken (Shimp, 1999). Perhaps, for example, we should consider the possibility that local and global analyses complement each other, and that it makes perfectly good sense to ask, not which of the two analyses is universally correct for all important problems, but what each means and in what contexts each applies (see also Hawkes & Shimp, 1998; Hineline, 2001; Williams, 1991). My reading of Staddon's book suggests he, too, sees potential value in both kinds of analysis. According to the Kuhnian view Baum endorses, however, it is presumably more difficult for him, because he sees himself as a participant in a paradigm clash between local and global analyses, to be as supportive of both.

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